DISCUSSION.

Professor Baldwin's Method of Studying the Color-Perception of Children.

A theory may be right or wrong, no matter whether there are many or few facts in support of it. On the other hand, a theory is right only when it can stand the brunt of a very large number of facts. If there are in the second case a few outlying 'facts,' the theory may still be right, because these facts may have been badly observed. If the facts on which the theory is based are very few or are equivocal, *i. e.*, differently interpreted by different observers, then the theory, though it may serve a man much in his own work, can take rank only as an individual hypothesis.

Professor Baldwin has lately published several theories in the domain of individual and child psychology. All of these theories have succeeded in getting themselves discussed; but no one of them, so far as I am aware, has got itself generally accepted. The general attitude of criticism seems to be that the theories are ingenious, and of the kind that would *a priori* carry conviction. But for this very reason they may be especially dangerous: first, because the uncritical mind may take for explanation what is only a working hypothesis; and secondly, because their schematic and suggestive exposition will make it very difficult for detailed criticism to reach them, the author being able to shift his ground and to modify his formulation at every attack. Still criticism must be passed, if only to check unconsidered acceptance; and serves its purpose if it rouses the author of the theory to a clearer, more definite and more comprehensive statement of his position.

Perhaps no theories are more likely to obtain credence than those which purport to lay a foundation for the science of education. While there are among educationalists some really careful thinkers, there are on the other hand, as one would naturally expect, thousands of industrious, thoroughly earnest and altogether untrained enthusiasts who have neither time, education, nor inclination carefully to weigh the multi-

tude of theories which are offered them, and which they eagerly desire to apply. Hence the fact that a psychologist in good standing has formulated and published a theory is not infrequently considered a sufficient evidence of its truth. Resulting from this comes a tendency to ignore all consideration of the theory itself, and to attempt at once its verification. But if theories are to be advanced with the abandon suggested by Prof. Baldwin, who says: "Give us theories, always theories! Let every man who has a theory pronounce his theory[1]!"—then surely the authors of these wholesale productions must expect them to receive especially critical examination, as theories, before their application is thought of.

Among other views that, it seems to me, must be expressed by his readers and considered by the author, before it can place itself on a practical working basis, I submit some of my own concerning Prof. Baldwin's theory of the law of suggestion or dynamogenesis as demonstrated in his experiments by the distance method upon the color perception of his child. They are the result of an honest study, undertaken primarily with the object of understanding the theory and its method.

The law of nervous dynamogenesis is briefly expressed in the statement that every state of consciousness tends to realize itself in an appropriate muscular development.[2] Suggestion in general is the tendency of a sensory or ideal state to be followed by a motor state.[3]

Prof. Baldwin has believed this for some time, and upon it has based the theory that the development of mind can be profitably studied. In 1890-1893 he published a series of articles in *Science* reporting observations on infants, and more recently (1895) in his book called "Mental Development in the Child and the Race: Methods and Processes," has gathered these together and attempted to synthetize his theory with that of the "current biological theory" of organic development. Along with the theory Prof. Baldwin has developed its application. In Chapter II of this book he explains his new method of child study, which is exceedingly attractive and very simple. He uses the color question to make clear his method, and says in this connection: "Only when we catch the motor response or a direct reflex, in its simplicity, is it a true index of the sensory stimulus in its simplicity." He thinks that hand movements are the most

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 38.

[2] "Handbook of Psychology: Feeling and Will," p. 281.

[3] "Distance and Color-Perception by Infants," *Science*, XXI, pp. 231, 232.

nearly ideal in this respect. Sensations, he says, are stimuli to movement; and the child's efforts with its hands become indications of the relative degree of discrimination, attractiveness, etc., of the different sensations which call the efforts out.[1] Among others mentioned he thinks that the following questions might be taken up by this method:

(1) "The presence of different color sensations, as shown by the number and persistence of the child's efforts to grasp the color.

(2) The relative attractiveness of different colors, measured in the same way.

(3) The relative attractiveness of different color combinations.

(4) The relative exactness of distance estimation, as shown by the child's efforts to reach over distances for objects,"[2] etc., etc.

The first investigator to attempt any systematic experiments on children as to color sensation was Prof. Preyer, who, in his well-known work *Die Seele des Kindes*, published in 1881, gives the results of 1,486 judgments of color made by his child. The experiments began at the end of the second, and continued almost to the end of the third year. Prof. Preyer's problem was to ascertain what colors the child could distinguish and rightly name. It was an investigation of color discrimination. His method involved knowledge by the child of the names of the colors. This knowledge Prof. Preyer gave him by showing him a few colors and teaching him their names. Then with the colors still before the child, he asked him: Where is the red? Where is the green? etc., requiring the child to point out the color. Or—and this method he seems to have followed for the most part—Prof. Preyer would say: What color is this? requiring the child to name the color. From time to time during the course of the experiments new colors were added. At the end of the 34th month the results were as follows[3]:

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 45.

[2] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., pp. 45, 46.

[3] *Die Seele des Kindes*, 4th ed., p. 121.

	Judgments.		Per Cents.	
	Right.	Wrong.	Right.	Wrong.
1. Yellow	232	8	96.7	3.3
2. Brown	79	8	90.8	9.2
3. Red	235	36	86.7	13.3
4. Violet	139	24	85.3	14.7
5. Black	39	7	84.8	15.2
6. Rose	76	29	72.4	27.6
7. Orange	47	23	67.1	32.9
8. Gray	35	33	51.5	48.5
9. Green	101	123	45.0	55.0
10. Blue	61	151	28.8	71.2
Total	1044	442	70.3	29.7

The next investigator to produce a new method was M. Alfred Binet. It is fitting, however, to speak, in the first place, of the experiments of Miss Milicent Shinn, who followed in the main Prof. Preyer's method, experimenting upon her niece in the third quarter of the child's second year. Here are her results:[1]

	Judgments.		Per Cent.	
	Right.	Wrong.	Right.	Wrong.
1. Pink	35	0	100	0
2. Orange	170	1	99.4	.6
3. Black	108	1	99.1	.9
4. Green	311	8	97.5	2.5
5. Yellow	240	7	97.2	2.8
6. Blue	309	22	93.4	6.6
7. Brown	11	1	91.7	8.3
8. White	117	11	91.4	8.6
9. Violet	63	10	86.3	13.7
10. Red	76	52	59.4	40.6
Total	1440	113	92.7	7.3

[1] "Notes on the Development of a Child" (University of California Studies), p. 49.

Prof. Preyer's child showed a tendency to confuse green and blue, which consequently appear at the end of his list; while the little girl subject of Miss Shinn became early uncertain regarding blue and red, with the result that red is tenth on her list. Yellow Preyer's child liked and discriminated best; and though it ranks fifth on her list, Miss Shinn is inclined to think it the favorite color of her niece. A comparison of the two tables, however, does not enlighten us to any great extent as to the probable tendency of color discrimination in young children. Much of the variation is probably due to word confusion.

M. Binet, realizing this, and also realizing that colors might be distinguished before word association is established, tried to obviate the difficulty by what he called the *méthode de reconnaissance.*[1] Preyer's method he calls the *méthode d'appellation.* Prof. Baldwin says that Binet, under the *méthode d'appellation,* "varied the conditions by naming a color and requiring the child to pick out the corresponding color," intimating that this was a new departure. Indeed, he says that "this gave results different not only from Preyer's, but also from those which Binet reached by Preyer's method."[2] But Preyer's method included *both* the naming of the color by the child *and* the pointing out of the color which was named. Sometimes he took the one way, sometimes the other. Binet's departure was the separation of the two. After carrying on both processes side by side for a while, as Preyer himself had done, Binet separated them, and kept a record of each. In one series he required the child to name the color; in the other to point out the color named. He made altogether by the *méthode d'appellation* 508 tests, which, arranged in percentages of right judgments, average as follows: red, 99; blue, 96; orange, 93; maroon, 86; rose, 74; violet, 71; green, 68; white, 54; yellow, 46. These results are obtained by averaging according to the number of tests given. Since these varied greatly (from 135 for red to 15 for orange), it would, perhaps, be fairer to find the percentage of right judgments in each of the three ways employed; *i. e.*, first, before the processes were separated; secondly, when the child named the color; thirdly, when she pointed to the color named: and to average these several results. The figures then are slightly different. Blue equals red and violet equals green; otherwise there is no change: red and blue, 99; orange, 93; maroon, 86; rose, 74; violet and green, 71; white, 54; yellow, 46. Binet's experiments

[1] *Revue philosophique,* Vol. XXX, pp. 583 ff.

[2] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 40.

were made upon his little girl, beginning when she was 32 months old, and continuing for a little over 6 months.

These, then, are the three tests made by Preyer's method, and they all differ widely. Yellow, so readily distinguished by Preyer's child, is by Binet's the least readily recognized. Red and blue, confused by Miss Shinn's niece, give almost no difficulty to Binet's little girl.

The results obtained by dividing Preyer's method are also interesting. When Binet's child said the name of the color, the order in percentages of right judgments is this: orange and blue, 100; red, 97; green, 83; maroon, 75; violet, 70; rose, 64; white, 33; yellow, 30. When the child indicated the color, the name being given her, the colors range themselves in this series: red, 100; maroon, 94; blue, 92; rose, 89; orange, 86; violet, 71; white, 68; green, 66; yellow, 58.[1] Binet by this clearly proves that the word element does affect the child's judgment. In order to test this still more closely, he carried on a series of experiments at the same time as the others by what he calls the *méthode de reconnaissance*. "I wanted to see," he says, "if this same child could find a color, which, after being shown to her, was mixed with several others. . . . . The *méthode d'appellation* places the child in rather artificial conditions. It is obliged to learn the names of the colors; it is forced to form a rational perception. It is evident that the child left to itself does not recognize colors by their names, but by visual memory; and the *méthode de reconnaissance* places it in more natural conditions than the other method."[2]

Prof. Baldwin, in discussing these methods, says: "It is, perhaps, a confirmation of Lehmann's position[3] that the

[1] In all calculations, .5 per cent. or more I have counted as 1 per cent.

[2] *Revue philosophique*, Vol. XXX, p. 589.

[3] Dr. Lehmann has an article in the *Philosophische Studien*, Vol. V, pp. 96-150, in which he shows that various grays upon rotating discs are recognized, if given names. Prof. Baldwin, in the "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 42, speaks of Lehmann as using "colored wools;" but gives no reference. In his "Hand-book of Psychology: Senses and Intellect," p. 177, he says: "As soon as some sign is made of a peculiar kind in connection with the image, it is recognized. Recent experiments by Lehmann on the recognition of differences of color strikingly confirm this view." The reader is referred to Lehmann, *Philosophische Studien*, Vol. VI. But Lehmann has no article in that volume. The volume referred to is probably V, in which appears Lehmann's *Ueber Wiedererkennen*. Dr. Burnham, in the AMERICAN JOURNAL OF PSYCHOLOGY, Vol. II, p. 610, referring to this article, says that Lehmann experimented with sensations of color, using different shades of gray produced by means of rotating discs. Both Prof. Baldwin and Dr. Burnham speak of Lehmann as

colors least recognized in Binet's list are shades whose names are less familiar to children; his list in order of certainty of recognition is red, blue, green, rose, maroon, violet and yellow, by the *méthode d'appellation*; and by both methods together, red, blue, orange, maroon, rose, violet, green, white and yellow."[1] In a footnote he says that the calculations were made from Binet's detailed results (*Revue philosophique*, 1890, pp. 582 ff.) by Mr. Tracy, and cites the latter's book, "The Psychology of Childhood."

It is not easy to see how Prof. Baldwin gets these results from Mr. Tracy's figures. For example, Mr. Tracy does not give any computations for the *méthode de reconnaissance*; and even if he had, there could have been no orange in the list of colors, as Binet did not make use of orange in the tests by this method. Prof. Baldwin, as will be seen, gets a certain order for the *méthode d'appellation*, in which orange is wrongly omitted: Mr. Tracy is probably responsible for this. But in the series Prof. Baldwin gives of both methods together, *i. e.*, the *méthode d'appellation* and the *méthode de reconnaissance*, the orange appears. Where he gets it, is not apparent. Mr. Tracy himself has interpreted Binet's tables for the *méthode d'appellation* very curiously, sub-dividing each series into a first and second series. The tables show no warrant for this. Mr. Tracy's results, therefore, wrought out in this peculiar manner, are confusing.

Prof. Baldwin criticises all of these methods and points out that even Binet's *méthode de reconnaissance* does not get rid of word association, since probably those colors are most easily recognized whose names are best known. It is here that he cites Binet's list. Taking the series that Prof. Baldwin gives of both methods together: red, blue, orange, maroon, rose, violet, green, white and yellow,—if we share his views we shall have to say that white is a less familiar name to a child than violet, and that green is a stranger word to him than maroon.

No other psychologist, I think, will agree with Prof. Baldwin in his position on the several points just discussed; but, on the other hand, none will fail to agree with him as to the general inadequacy of the methods so far advanced for the

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experimenting with colors; whereas, in reality, he worked only with *grays*. It may be that Prof. Baldwin, having made this first mistake, slipped naturally into the second one,—the use by Lehmann of wools. Neither in *Ueber Wiedererkennen*, nor in anything else that Lehmann has written, can I find any evidence of his having experimented with colored wools.

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 42.

investigation of color perception in children, and the need for a better one.

Prof. Baldwin now comes forward with a new method, based on the law of dynamogenesis referred to above.[1] He thinks that the young child's reaching movements reflect its sensibility. The two variables to be considered in this method are the quality of the stimulus and its distance from the child. The drawing-out influence of the stimulus will vary with the quality and in inverse ratio with the distance of the colored object. While Prof. Preyer's experiments were directed upon color discrimination, and M. Binet's by the *méthode de reconnaissance* upon color recognition, Prof. Baldwin says: "I undertook at the beginning of my child H.'s ninth month to experiment with her with a view to arriving at the exact state of her color perception, employing this new method." Bits of colored blotting paper were placed at different distances from the child, one at a time. Account was kept of the number of times she reached out toward each. The colors were blue, red, white, green and brown. Newspaper (presumably cut to the same shape and size as the colored pieces of blotting paper) was used as "a relatively neutral object." Yellow unfortunately was not used, as Prof. Baldwin could not obtain a yellow in his neighborhood that suited him. Since the experiments extended over a period of six months, it is difficult to understand why he did not have recourse to some other neighborhood. Below are the tables showing the results of Prof. Baldwin's experiments, as they appear in the second edition of "Mental Development in the Child and the Race: Methods and Processes."[2]

R is the symbol for refusal, A for acceptance. N stands for the entire number of experiments with each color respectively, and n for the entire number with all the colors at each distance respectively. $\frac{A}{N}$=the proportion of acceptances or efforts for any color, and

$\frac{R}{n}$=the proposition of refusals for each distance.[3]

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[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., Chap. II.

[2] See p. 52.

[3] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 54.

TABLE I.

DISTANCE, INCHES.	9	10	11	12	13	14	15	TOTALS.	RATIO $\frac{A}{N}$
	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A. N.	
Blue	0—1	0— 4	0—5	1— 3	2— 4	1— 5	3—1	7— 23— 30	.766
Red	0—1	0— 3	2—2	1— 4	1— 7	1— 7	5—1	10— 25— 35	.714
White	0—0	0— 0	0—0	0— 1	0— 5	1— 1	3—0	4— 7— 11	.633
Green	0—0	0— 1	0—1	2— 1	1— 4	1— 2	2—0	6— 9— 16	.60
Brown	0—1	0— 2	2—1	3— 2	0— 3	3— 1	2—0	10— 10— 20	.50
Totals	0—3	0—10	4—9	7—11	4—23	7—16	15—2	37— 74—111	.66
Ratio $\frac{R}{n}$	0	0	.30	.39	.15	.30	.90	Total, .33	

TABLE II.

DISTANCE, INCHES.	9	10	11	12	13	14	15	TOTALS.	RATIO $\frac{A}{N}$
	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A.	R. A. N.	
Newspaper				0—17	0—28	1—33	25—2	26— 80—106	.76
Color	0—3	0—10	4—9	7—11	4—23	7—16	15—2	37— 74—111	.66
Totals	0—3	0—10	4—9	7--28	4—51	8—49	40—4	63—154—217	.71
Ratio $\frac{R}{n}$			.30	.20	.07	.14	.91	Total, .29	

Despite the fact that this is the third time of their printing, the tables still contain 7 errors in calculation. In Table I, under the sub-head N of column headed *Totals*, 16 should

read 15. Under *Ratio* $\frac{A}{N}$ for green .633 should read .636. *Ratio* $\frac{R}{n}$ for 11 inches should be .307, not .30. (It ought consistently to read .31, since in the next column .388 is made .39, and in that following .148 is put down as 1.5). Under 15 inches *Ratio* $\frac{R}{n}$ should be .882, not .90. In Table II *Ratio* $\frac{R}{n}$ for 11 inches, .30 again should read .307 or .31, not .30. *Ratio* $\frac{A}{N}$ for newspaper should be .754, not .76, and for colors .666, not 66.

As first printed in *Science*, April, 1893 (leaving out of account some less considerable inaccuracies of fractional percentages), the tables contain the following errors: In Table I under the sub-head *R* in the column *Totals* for green, 7 should be 6; under *N* 16 should be 15. Under the same headings for brown 11 should be 10, and 21 should be 20. Under column *Ratio* $\frac{R}{N}$ for green, .56$\frac{1}{3}$ should be .60, and for brown .47$\frac{1}{2}$ should be .50. In Table II *Ratio* $\frac{R}{n}$ for 11 inches should be .307, not .33$\frac{1}{3}$, and for 15 inches *Ratio* $\frac{R}{n}$ should be .882, not .89.

As printed in the first edition of "Mental Development in the Child and the Race: Methods and Processes," the tables contain 16 mistakes, ranging in error of percentage from 2 to 15. In the preface to the second edition, the author says: "The demand for a new edition of my book gives me the opportunity to make certain minor corrections throughout. The only important alteration is to be found in the tables (I and II) on p. 52, in which certain columns had been substituted from other tables which lie unpublished among my papers." Nevertheless, recalculation has disclosed the seven errors referred to above.

These facts are significant in themselves: for a writer who is careless in his presentment of facts we shall incline to suspect of being careless in his interpretation. Indeed, Prof. Baldwin's interpretation of these tables is as puzzling as the tables themselves. I have been able to understand it only on the assumption that *some of his statements in the second edition are based upon the figures of the first edition* of his book.

(1) His first statement is that "the colors range themselves in an order of attractiveness, *i. e.*, blue, white, red, green and brown." While this is a possible interpreta-

tion of the tables of the first edition, a glance at column *Ratio* $\frac{A}{N}$ shows that red should rank next to blue. In the *Science* article, "Distance and Color Perception by Infants," Apr., 1893, the interpretation is given correctly. It would seem, then, that Prof. Baldwin interpreted from the 'substituted' tables of the first edition, where *Ratio* $\frac{A}{N}$ reads: blue, .78; red, .75; white, .78; green, .68; brown, .50.

(2) White, he says, was more attractive than green, and slightly more so than red. But, according to the figures of the second edition, white is .08 *less* attractive than red. This misstatement also may have been caused by reading from the incorrect table.

(3) The next sentence says: "The newspaper was at reaching distance (9-10 inches) and a little more (up to 14 inches) as attractive as the average of the colors, and even as much so as the red."[1] We *are not given the figures* for newspaper at 9, 10 and 11 inches; but beginning at 12 inches and going up to and including 14 inches, we find that newspaper is *far more attractive* than any color tested, even than blue. The proof is obvious: *Ratio* $\frac{R}{n}$ (proportion of refusals) at 14 inches, for blue is .166, for red is .15, for white is .50, for green is .33⅓, for brown is .75, and for newspaper is .029; while *Ratio* $\frac{R}{n}$ for newspaper at 12 and 13 inches is 0.

(4) The attractiveness of the newspaper in these distances Prof. Baldwin accounts for by the fact that "the newspaper experiments came after a good deal of practice in reaching after colors, and a more exact association between the stimulus and its distance." Reaching distance, we are told, was 9 to 10 inches. If the child was improving in her estimation of distance, one would expect her, at a distance of 14 inches, to begin to refuse the newspaper, simply because she knew that she could not reach it; yet she does so in only 1 case out of 34, while the colors are refused at 14 inches in 7 out of 16 cases. The explanation, too, that these newspaper experiments came after a "good deal" of practice in reaching, is not altogether satisfying. Since a careful record was kept of each test, it would have been easy for Prof. Baldwin accurately to state just how much practice the child had received before the newspaper stimulus was offered her. Failure to state the conditions of his experiment on the part of an investigator must result in

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 53.

failure to accept his conclusions on the part of his readers. Prof. Baldwin could and should have given this condition.

(5) While the *attractiveness* of the newspaper up to 14 inches is accounted for by the child's practice in reaching, the *refusal* of the newspaper at 15 inches is accounted for, curiously enough, in the same way. "At 15 inches and over, accordingly, the newspaper was refused in more than 92 per cent. of the cases, while blue was refused at that distance in only 75 per cent., and red in only 83 per cent." Since the child had had "a good deal of practice" in reaching, and had established a more exact association between the stimulus and its distance, it is hard to account for the use of the word 'accordingly' in the above quotation. One might better attribute the refusal of newspaper at this greater distance to the dynamogenic power of color (and this Prof. Baldwin seems to do in citing the smaller percentages of refusals of blue and red), were it not for the fact that green, white and brown, respectively, are refused at this distance in 100 per cent. of the cases!

(6) Prof. Baldwin then calls attention to the striking fact that the child refused persistently to reach for anything put at 16 inches or more away from her, and at 15 inches refused 91 per cent. of all the cases, 90 per cent. (the correct figures are 88.2 per cent.) of the color cases, and 92 per cent. of the newspaper cases. This shows, he thinks, the very accurate visual estimation of distance acquired by the child. "The child's interpretation of the distance inhibits all effort to reach across it." These interpretations result, we are told, from "associations of visual indications of distance with sensations of hand and arm movement."[1]

The inclination to accept this result and its explanation, were there no other reasons, would be destroyed by Prof. Baldwin's failure to state conditions and his vague use of terms. For example, after giving on page 54 the above figures for distances of 15 inches and over (the explanation for them being presented on pp. 76-77),[2] Prof. Baldwin says: "At nearer distances we find the remarkable uniformity with which the *safe-distance* association works at this early age. At 14 inches only 14 per cent. of all the cases were refused, and at 13 inches only about 7 per cent." The natural inference here is that 14 inches is included in the '*safe-distance.*' But on page 77 he speaks of the *safe-reaching dis-*

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., pp. 76-77.

[2] The habit of later incidental explanation is characteristic of the book, and is very confusing.

tance; the *uncertain-reaching distance*, and the *impossible-to-reach* distance; and on page 49 he says that "in some cases the inhibition of *d* (distance) does not work, and the child oversteps all its experience in violent straining and tears." On page 53 he calls reaching distance 9 to 10 inches; and on page 54,again, says that there were no refusals to reach for anything exposed within reaching distance (10 inches). Such, then, are the various statements for us to attempt to reconcile.

If we include 14 inches within the *safe-reaching distance*, and put 15 inches in the category of *impossible-to-reach distance*, then *uncertain-to-reach distance* is excluded. Possibly 9 to 10 inches is intended to mean what we may term '*easy-reaching distance*,' and 14 inches that distance which calls forth "straining and tears;" but this does not explain *safe-distance*. If 9 to 10 inches is the distance beyond which the child cannot reach, and if this is also the *safe-distance*, then it is hard to see how the *safe-distance* association worked, as we are told it did, at 14 inches. If, on the other hand, 14 inches is the 'strain' limit of her reaching distance, and is included in the *safe-distance*, not only does Prof. Baldwin's *uncertain-reaching* distance disappear, but his use of the term "reaching distance" in other connections cannot be understood.

Leaving us in a state of uncertainty as to what he means by *safe-distance* association, Prof. Baldwin goes on to speak of the results obtained with objects displayed within 10 inches. He says: "The fact that there were no refusals to reach for anything exposed within reaching distance (10 inches)—other attractive objects being kept away—shows two things: (1) the very fine estimation visually of the distance represented by the arm-length, thus emphasizing the element of muscular sensations of arm-movement in the perception of distance generally; and (2) the great uniformity at this age of the phenomenon of 'sensori-motor suggestion' upon which this method of child study is based." Sensori-motor suggestion is here conditioned by three facts: (1) distance from the object, (2) its attainment or non-attainment, (3) its quality. That Prof. Baldwin is not unmindful of the second condition is shown by what he says in giving the formula of the dynamogenic method: "If the child reaches for a blue-*q* [q=quality] at 12 inches, and just gets it, he will then reach for a green-*q* with greater avidity at twelve inches than he would otherwise have reached for the same green-*q* at nine inches."[1] But

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 48.

again we are left in the dark. We are not told whether the child at "reaching distance" really succeeded in grasping the object or not. How important this condition is, the above quotation plainly shows. The only information we are given relating to this point is on page 56, where Prof. Baldwin says: "Further, after each effort or two the child should be given the object reached for to hold or play with for a moment; otherwise he grows to apprehend that the whole affair is a case of Tantalus." "After each effort or two" is not as definite as the statement should have been made. Presumably, however, the child was given the object very frequently, whether she was able to reach it or not. In that case, condition (2) (attainment or non-attainment of the object) is almost, if not entirely, ruled out so far as the experiments are concerned, and the child's refusal to reach for objects at 15 inches and over is so much the more striking. On the other hand, any irregularity in giving the child the object would tend to affect her reaching. This would also influence her perception of color. Unless great care were taken that each color secured the privilege of being held or played with, "for a moment," an equal number of times, there would be danger that those colors receiving oftenest this closer contact would become the most readily recognised. Again, an object played with might be reached for, and yet not possess in its color quality enough dynamogenic power to call out movement.

(7) He says that the fact that there was a larger percentage of refusals at 11 and 12 inches than at 13 and 14 inches is due to the influence of brown, "which was consistently refused when more than 10 inches away." If brown was consistently refused when more than 10 inches away, then it must have been refused at 11, 12, 13, 14 and 15 inches respectively, and one cannot understand how that accounts for there being more refusals at 11 and 12 inches than at 13 and 14 inches. Clearly it does not. But even if this were a reason, it could not be advanced; for a glance at the tables shows that brown was *not* refused at 13 inches, but was accepted every time. If it had been refused as often at 13 inches as at 11 and 12 inches, the percentage of refusals for 13 inches would have been much higher. There is a valid reason, it seems to me, that Prof. Baldwin might have brought out, and that is the small number of tests given at 11 and 12 inches for green and white. The total number of tests for green at 11 and 12 inches is 4, for white 1; while the total number for green at 13 and 14 inches is 8, and for white is 7.

(8) Newspaper taken as a "neutral object"—and we are

not at all sure that it *is* neutral to the child—is not put on a fair basis of comparison. The tables give us no tests for the first three reaching lengths. If they did, newspaper would presumably rank ahead of the colors in attractiveness; for the nearer distances give a preponderance of acceptances. Ten out of the 23 acceptances for blue come within 9, 10 and 11 inches with no refusals; 6 out of the 25 for red with only 2 refusals; 2 out of the 9 for green with no refusals (green was not tested at 9 inches); and 4 out of the 10 for brown with 2 refusals. Putting it differently: at 9 and 10 inches there are no refusals for the colors, and at 11 inches the acceptances for the colors collectively are 70 per cent. It is safe to infer that had it been tested at this range the *Ratio* $\frac{A}{N}$ for newspaper would have exceeded even that of blue. There is one other way to place newspaper on an equal footing with the colors, and that is to exclude the first three distances. If we do so, the stimuli range themselves in the following order: newspaper, .754 (given in the table .76); red, .703; blue, .65; white, .636 (given in the table .633); green, .538; brown, .444. White, like newspaper, was not tested at all in the first three distances, and green but twice.

The relatively small number of tests made at the nearer distances, 9, 10 and 11 inches (none at all, as has been said, being given for white, only two for green and none for newspaper), the wide variation in number of tests (ranging from a totality of 3 experiments for all objects at 9 inches to 57 at 14 inches), added to the fact (which Prof. Baldwin himself recognises) of the absolutely small number of experiments represented by the tables,[1] forbid any accurate comparisons between stimulus and stimulus, or any safe conclusions upon the results as a whole. Yet Prof. Baldwin has not hesitated to make both.

In *Mind*, Apr., 1896, he says that he was not testing color discrimination or color preference, but color perception. It would seem, then, that he is illogical when he compares his results with those of Preyer and Binet, as he does on page 53 of his book. "Disregarding white, the difference between blue and red is very slight as compared with that between any other two. This confirms Binet as against Preyer, who puts blue last, and also fails to confirm Preyer in putting brown before red and green. Brown to my child—as tested in this way—seemed to be about as neutral as could well be." One would certainly infer from this that Prof. Baldwin

[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 57.

thought that the child not only perceived the colors, but *discriminated* between them ; yet there is nothing in the experiments to tell us this. Moreover, Prof. Baldwin disclaims having investigated for discrimination. His comparisons, then, of his results with Preyer's, who did investigate for discrimination, are, to say the least, somewhat inconsistent.

He criticises Preyer because, he says, Preyer's results cannot be analysed ; yet his own are open to the same objection. He tells us that he experimented with his child in order to arrive at "the exact state of her color perception."[1] The interpretation of the tables, instead of giving us any information about "color perception," begins with this statement: "The colors range themselves in an order of attractiveness." The natural inference is that Prof. Baldwin considers 'order of attractiveness' and 'order of perception' as synonymous expressions. But surely it does not necessarily follow that because the child reached out proportionately a greater number of times for blue than for red, she perceived blue better than red. It may have been that the child had a feeling for blue *plus* the sensation, and that to this feeling was due the greater 'attractiveness' of blue. Brown, on the other hand, may have produced in her a feeling of repulsion, or may not have been noticed as color at all, and the motor response have been called out simply by the form of the object.

Prof. Sully in *Mind*, Jan., 1896, reviews "Mental Development in the Child and the Race: Methods and Processes." He suggests that at first the objects grasped at be some sort of uncolored playthings, and then that they be colored and used as stimuli. In this way the last objection made would be to some extent avoided. The difficulty, however, would be in determining upon an 'uncolored' object. Prof. Sully also suggests that we do not know how soon children are biased in their preference by the colors of their environment, the mother's dress, for instance.[2]

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[1] "Mental Development in the Child and the Race: Methods and Processes," 2d ed., p. 50.

[2] Since writing this article I have found another reference to Prof. Baldwin's method in Prof. Sully's "Studies of Childhood," pp. 20-21. He repeats again the statement made in *Mind* that the method does not test color discrimination, but color preference, and adds that, even as a test of preference, it is likely to be misapplied: if the colors are not equally bright, if one color falls more often into the first or fresh period of the experiment, or if one color be brought in after longer intervals of time than another.

The first point has been discussed in this paper. Prof. Baldwin says he was not testing color discrimination. The necessity of equality of brightness he himself calls attention to on page 56 of his

The tables do not prove, again, that the child perceived blue, red, white, green and brown as separate colors, or that certain of these were (on account of their peculiar color qualities) more attractive than others; because newspaper, the 'neutral' object, even under its unfair conditions was as 'attractive' as any colored stimulus.

Prof. Baldwin's new dynamogenic method, then, ingenious as it is in its conception, and attractive as it is in its simplicity, has, nevertheless, been so carelessly carried out by its author that no reliance can be placed in the results obtained. True, the idea still remains; and it is left, possibly, for some less brilliant but more accurate experimenter to demonstrate its worth.

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book. As to how he met the fatigue factor, no statements are given except that experiments were stopped if the child showed signs of weariness. Whether or not Prof. Baldwin considered the attractive force of novelty which would result from the fact that one color is presented after a longer interval of time than another we do not know. As has been said, we are given no information as to the regularity or irregularity with which the various colors were presented.